1884MNRAS..44..225S

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLIV. MARCH 14, 1884. No. 5.

EDWIN DUNKIN, F.R.S., President, in the Chair.

John T. Stevenson, Auckland, New Zealand, was balloted for and duly elected a Fellow of the Society.

On the Necessary Distinction in Practical Astronomy between the True Mean Solar Day and the Mean Day adopted, from time to time, in the Construction of our Astronomical Tables, and in the Comparison of these Tables with Observations. By E. J. Stone, M.A., F.R.S.

The papers which have appeared in connection with this question in the *Monthly Notices* for 1883, December, and 1884, January, show that the physical conditions of the problem are most imperfectly understood. We certainly have not two independent mean Suns, and therefore two independent real Suns, and therefore two independent positions of the observer at any given time available in practical astronomy to satisfy any analytical requirements; nor can we measure motion correctly in terms of a time which is itself a fallible quantity, whose errors increase as the time increases; and it is equally impossible for us, so long as we are ignorant of the value of the Sun's mean motion in longitude in the true mean solar day, to determine times from the motion of the meridian *alone* expressed in terms of a unit whose length itself depends upon the *relative* motions of the meridian over that of the Sun in mean longitude; for the two motions are perfectly independent. We have no doubt attempted to do this; and theoretical astronomers have supposed that we were really using the *true* mean solar day as our unit; and it is because we have attempted to do what is physically impossible, and have directly introduced into our

1884MNRAS..44..225S

determinations of time important conditional equations which would be true if we *were* using the true mean solar day as our unit, but which are not true in practice, that we have introduced discordances between our theories and observations which have led to the necessity of such hypotheses as "changes in the time of the rotation of the Earth on its axis," or "unknown physical actions," and have wasted the energies of some of our best men and squandered thousands of pounds.

It is absolutely impossible for us to determine motions in terms of a variable t which is not quantitatively referred to some continuous motion which is itself measurable, or to some such difference between motions; and the equation which connects the standard motion and the variable t, and from which t is determined, is, of necessity, subject to the conditions of its use in practice, *an exact equation*. It is this equation, subject to its use in practice, and not any theoretical views of our own, which fixes the scale upon which our times are measured, or the unit of time which is adopted in practical astronomy. But the observer's position depends upon two perfectly independent motions—the motion of the Earth about its centre of gravity and the motion of that centre of gravity itself. If the symbols ω and n denote the motion of the Earth about its axis, and the mean motion of the centre of gravity of the Earth in longitude respectively, then, before we can compare our theories of the motions of the centres of gravity of the Earth, Moon, and planets with observations, or determine the constants involved in our theoretical expressions, we must establish some relationship between ω and n; but, since the motions of ω and n are perfectly independent of each other, we can only establish such a relationship indirectly through T, some interval of time which we adopt as our unit. With every variation of T, ω and n will both vary, but so long as the symbols represent the physical quantities which I have indicated, and when they no longer indicate these physical quantities, we have no concern with them, the variations must satisfy the equations

$$(1)\quad \frac{\delta\omega}{\omega} = \frac{\delta n}{n} = \frac{\delta \mathrm{T}}{\mathrm{T}}.$$

If we will introduce in our practical determinations of mean times any conditional equations depending upon the length of the *true* mean solar day, or any other similar physical conditions, we must employ an *exact equation* between ω, n, t, or we shall necessarily introduce secular separations between the two motions which fix the observer's position; but if t denote the length of the true mean solar day expressed in terms of any unit T, we have identically, from the definition of the mean solar day,

$$(2)\quad \omega = \frac{360 \times 60 \times 60}{t} + n.$$

1884MNRAS..44..225S

And if the symbol T_0 denote the length of the true mean solar day, we shall have

$$t\,.\,T = T_0,$$

and therefore

$$(3)\quad \omega = 360 \times 60 \times 60 \times \frac{T}{T_0} + n.$$

This equation (3) is true for all corresponding values of ω, T, and n, and is exact, but differentiating we have

$$\delta\omega = 360 \times 60 \times 60 \times \frac{\delta T}{T_0} + \delta n,$$

or since

$$\frac{\delta T}{T} = \frac{\delta n}{n},$$

we have, if $T = T_0$ nearly,

$$\delta\omega = 360 \times 60 \times 60 \times \frac{\delta n}{n} + \delta n.$$

This equation establishes the only possible law of variation between ω and n when ω and n denote the physical quantities of our problem. If, therefore, n_0 is the true value of the mean motion in longitude of the centre of gravity of the Earth in the true mean solar day T_0, we have

$$\omega_0 = 360 \times 60 \times 60 + n_0\,;$$

but, when we adopt n instead of n_0, the true expression is not

$$\omega = 360 \times 60 \times 60 + n,$$

as astronomers have hitherto assumed, but

$$\omega = 360 \times 60 \times 60 + n_0 + 360 \times 60 \times 60 \frac{n - n_0}{n_0} + n - n_0.$$

or

$$(4)\quad \omega = 360 \times 60 \times 60 + n + 360 \times 60 \times 60\, \frac{n - n_0}{n_0},$$

This equation is exact. The omission of the term

$$360\,.\,60\,.\,60\,.\,\frac{n - n_0}{n_0},$$

would at once, with times determined with the erroneous value of ω, introduce secular errors into the theoretical positions of the Sun, Moon, and planets. If, therefore, s denotes the sidereal time, we have

$$s = a + 24^{\mathrm{h}}\,.\,t_0 + \frac{n_0}{15}\,.\,t_0,$$

1884MNRAS..44..225S

if t_0 is the true mean solar time; but we have with equal exactness

$$s=a+\left(1+\frac{n-n_0}{n^0}\right).t+(n_0+n-n_0).t,$$

or

$$(5)\quad s=a+\left(24^{\mathrm{h}}+n+24^{\mathrm{h}}\frac{n-n_0}{n_0}\right).t,$$

when we adopt n for the Sun's mean motion. The only difference is that our time now is not *true mean solar time*, but mean time referred to some definite unit T.

If, therefore, we attempt from a given observed value of s to infer the true sidereal interval from mean noon by the formula usually adopted,

$$s=(a+nr),$$

where r is the number of complete days from epoch, we commit the error

$$-24^{\mathrm{h}}.\frac{n-n_0}{n_0}.r.$$

But if we are using $n+\delta n$ instead of n we commit the error

$$-24^{\mathrm{h}}\left(\frac{n+\delta n-n_0}{n_0}\right).r.$$

The difference between these errors is a determinate quantity,

$$-24^{\mathrm{h}}\frac{\delta n}{n_0}.r.$$

The question is, Can we neglect this difference? The answer is emphatically, No! I have proved this by a direct numerical calculation. It accounts for the existing large discordances between the lunar tables and observations, and such terms cannot therefore be neglected as insensible.

If we start from the given sidereal time of observation, as we do in practice, these results show that, instead of taking out our tabular places for the same noon before and after the change from n to $n+\delta n$, we must correct all our tabular places for an error of time

$$24^{\mathrm{h}}\frac{\delta n}{n}.r,$$

in order that they may correspond to the new mean noon as they did correspond to the old mean noon. The difficulty which appears to be felt in this matter is probably due to overlooking the fact that if we commit such systematic errors in referring our sidereal times to our mean times, as I have proved we do commit so long as n_0 is unknown, we determine in practice not the true mean solar times, but other times; and we express, as the only times available to us, all our mean motions

1884MNRAS..44..225S

in terms of these times, and not in terms of the true mean solar times as we have hitherto supposed. A change in the adopted value of n, therefore, necessitates a redetermination of all our mean motions, or a correction to the times such as I have applied; for when we determine our times directly from the motion of the meridian with any adopted value of ω, *all errors in our time determinations are thrown directly upon the theoretical expressions for the motions of the centres of gravity.*

This explains why Prof. Adams, in attempting to show that no errors have been made in these questions of tabular times, is driven to assert that we have nothing to do with the Right Ascension of the meridian at mean noon; and that my views must be wrong because the adopted tabular expressions for the motions of the centres of gravity are made to represent the same motions in the same absolute times before and after any arbitrary changes which we may be pleased to adopt in our time determinations. The way in which these small errors accumulate is no doubt difficult to get a firm grasp of. And perhaps the point may be put somewhat more simply as follows.

If we adopt a value n_1 for n, then the unit of time is certainly fixed and determinate. Let this value be T_1, and let ω_1 be the value of ω expressed in terms of T_1, and t_1 the length of the true mean solar day, then clearly

$$t_1 = \frac{360^\circ}{\omega_1 - n_1}.$$

And of n_2, T_2, ω_2, and t_2 are other corresponding values,

$$t_2 = \frac{360^\circ}{\omega_2 - n_2},$$

and

$$\frac{t_1}{t_2} = \frac{\omega_2 - n_2}{\omega_1 - n_1}.$$

But since these equations are exact, we have

$$\frac{\omega_2 - n_2}{\omega_1 - n_1} = \frac{T_2}{T_1} = \frac{n_2}{n_1}.$$

We have, therefore,

$$\frac{t_1}{t_2} = \frac{n_2}{n_1},$$

or

$$\frac{t_1 - t_2}{t_2} = \frac{n_2 - n_1}{n_1}.$$

It is, therefore, perfectly impossible for us to use the true mean solar day as our unit when we are continually changing the adopted values of n. But the question practically under consideration is, What will the errors of the false assumption amount to? The equation

$$\frac{t_1 - t_2}{t_2} = \frac{\delta n}{n}$$

1884MNRAS..44..225S

answers the question. We shall make an error of

$$\frac{\delta n}{\ } \times \text{days measured.}$$

The change thus made in 1864 accounts for the large increase of the discordances between Hansen's tables of the errors and observations, and the errors of the false assumption, therefore, cannot be neglected. But it may be said that astronomers have proved that the changes in the units of time due to changes from n to $n+\delta n$ in the adopted values of the Sun's mean motion in longitude are not in the ratio of $n : n+\delta n$, as assumed, but in the ratio of $\omega : \omega+\delta n$. Astronomers have certainly proved nothing of the kind, and it is perfectly impossible that any such result can be true when ω and n represent the physical quantities which I have made them represent. Prof. Cayley has proved that if we have two Suns—or two Earths, rather—moving with mean motions in longitude of n and $n+\delta n$ respectively in the adopted unit of time, then the mean solar days would differ by nearly

$$\frac{\delta n}{\omega} \times \text{length of a day.}$$

Prof. Cayley's result is of course perfectly correct on the assumptions made; but in practical astronomy the observer's position is fixed through the n and the ω, and n expresses the mean motion of the centre of gravity of the Earth in longitude. The n and $n+\delta n$, therefore, refer to the motion of the same physical point, and not to two independent moving points, as Prof. Cayley supposes in his problem. The result given by Prof. Cayley, therefore, does not prove that

$$\frac{\delta \mathrm{T}}{\mathrm{T}} \text{ is not } = \frac{\delta n}{n}$$

when the symbols have the physical meanings which I have assigned to them. As a matter of fact, Prof. Cayley obtains my result when restricted to a single moving point.

Prof. Adams has also shown that, subject to certain conditions,

$$\frac{\delta \mathrm{T}}{\mathrm{T}} \text{ is equal to } \frac{\delta n}{\omega}.$$

And this result is equally true on the assumptions made, but they are not those of the physical problem under consideration, as we can easily show. If we attempt to establish a relation between ω and n by successive approximations through ω, starting from $\omega = 360°$ in the sidereal day S, we can do this quite accurately provided we introduce no physical conditions which the symbols ω, n, and T are made to satisfy in our practical determinations of the time during the process of approximation. For, putting

1884MNRAS..44..225S

$\omega = 360° + n$ and $360° + n + \delta n$ respectively, we have for the sidereal time respectively

$$s = a + \left(24^{\text{h}} + \frac{n}{15}\right)t$$

and

$$s = a + \left(24^{\text{h}} + \frac{n + \delta n}{15}\right) . t^1$$

and therefore

$$= t^1 \left(1 + \frac{\delta n}{360° + n}\right).$$

But this result is utterly inconsistent with the assumption which is always made in practical astronomy that the true mean solar day is always used as our unit, and that we can therefore neglect the difference between t and t' in the large terms $24^{\text{h}}t$ and $24^{\text{h}}t'$, and thus write

$$24^{\text{h}} . t = 24^{\text{h}} . t'$$

whenever t' is an integer; this requires that

$$s = a + \frac{nt}{15} = a + \frac{(n + \delta n)t'}{15},$$

and therefore instead of

$$t = \left(1 + \frac{\delta n}{\omega}\right) . t',$$

$$t = \left(1 + \frac{\delta n}{n}\right) . t',$$

to satisfy the necessary conditions.

Prof. Adams's result is therefore not true, as we use the expressions

$$360 \times 60 \times 60 + n$$

and

$$360 \times 60 \times 60 + n + \delta n$$

in our practical determinations of time.

This point is rendered clearer by returning to the accurate equation which I have proved,

$$s = a + \left(\frac{360 \times 60 \times 60}{15} . \frac{\text{T}}{\text{T}_0} + \frac{n}{15}\right)t.$$

For since $t\,\text{T} = t_0\,\text{T}_0$, identically, if t_0 denote the true mean solar time, we may write this equation under the form

$$s = a + 24^{\text{h}}\, t_0 + \frac{n}{15} . t.$$

If we change n into $n + \delta n$, since

$$(\text{T} + \delta\text{T})t_1 = t_0\text{T}_0,$$

1884MNRAS..44..225S

we have

$$s = a + 24^{\text{h}}\, t_0 + \frac{n + \delta n}{15} \,.\, t$$

These equations show at once that if we always suppose the time measured in true mean solar time in the principal term 24^{h}, we simply throw the adjustment of the mean times to the given sidereal times upon the small terms. If we determine our mean solar time, firstly, from the equation

$$s = 24^{\text{h}} \,.\, t_0 + \frac{n}{15} \,.\, t,$$

secondly, from the equation

$$s = 24^{\text{h}} \,.\, t_0 + \frac{n + \delta n}{15} \,.\, t',$$

then the times must differ in the proportion of the units $n : n + \delta n$. And if we write these expressions

$$s = a + 24^{\text{h}} \,.\, t + \frac{n}{15} \,.\, t$$

and

$$s = a + 24^{\text{h}} \,.\, t + \frac{n + \delta n}{15} \,.\, t,$$

and simply assume that the mean noons are the same in both cases, so that we can put $t = r$ = number of true mean solar days at true mean noon, which comes to the same thing as writing

$t = t_0$ in the principal term,

we must find the values of t as before expressed in terms of units in the proportion of $n : n + \delta n$.

All I have done, therefore, is to simply transfer Le Verrier's expression so as to make it give identical results with those which would have been obtained by the direct use of Bessel's expression unaltered since 1864.

This leads to an important remark and question of Professor Cayley's in the January number of the *Notices* to which I have not yet alluded. Professor Cayley says (p. 85): "Mr. Stone, as I understand him, contends that for the same instant of time the two points (i.e. really the observer's position) are one and the same point." If so, it seems to me that Hansen's formula (or Le Verrier's) must be a mere analytical transformation of Bessel's, and the question would arise, Why was such a change made?" The change made in our expression for the sidereal times *was quite unnecessary*. It was done no doubt because it was supposed that the value of the Sun's mean motion deduced from a discussion of the observations in terms of the adopted unit of time was more accurate than that adopted by Bessel, and it was not known apparently that by adopting this value in the expression for the sidereal time in terms of the solar time we

1884MNRAS..44..225S

should necessarily change the adopted unit. But, whatever may have been the reason for the change, there is only one way of estimating its effects, and that is, to transfer the variable t' of Le Verrier (or Hansen), as Professor Cayley practically admits I have done, into that of the t of Bessel. This, at all events, gives correctly the effects of the change upon existing tables. What we want is to separate the discordances between Hansen's tables and observations into two parts—one due to our arbitrary action in 1864, and the other due to errors in the tables themselves. As some serious misconceptions appear to exist upon the point, it should be known that Le Verrier deduced the correction δn to the adopted value n from a discussion of the observations referred to the meridian with the condition

$$s = a + \left(24^{\text{h}} + \frac{n}{15}\right)t + \text{\&c.};$$

and therefore the value $n + \delta n$, so far as the correction δn was really required, was only true when this relationship existed between s and t; when, therefore, after the construction of the tables, we adopted

$$s = a + \left(24^{\text{h}} + \frac{n + \delta n}{15}\right).t',$$

and assumed at every mean noon, instead of

$$sn = a + \frac{n}{15}.t,$$

that

$$sn = a + \frac{n + \delta n}{15}.t',$$

we actually introduced the error

$$(n + \delta n)t' - nt,$$

or, if t' is supposed to be equal to t, the error

$$\delta nt,$$

or, practically, sensibly doubled the correction δn which Le Verrier thought necessary to reconcile theory and observation with the old unit of time.

With respect to the inexactness of such equations as those connecting s and t: they are used in two ways, in both of which they must be exact; we determine from such equations the mean time from a given sidereal time, or we eliminate by the aid of this equation the variable t, and thus find the tabular positions of the Sun, Moon, and planets on the meridian for direct comparison with our observations.

The following is perhaps the simplest form in which the proof of the change can be put.

1884MNRAS..44..225S

If

$$(\alpha)\quad s = a + \left(24^{\mathrm{h}} + \frac{n}{15}\right)t$$

is Bessel's expression, I *can* certainly transfer t into t' time as follows. Let

$$nt = (n + \delta n)t',$$

then

$$(\beta)\quad s = a + \frac{n+\delta n}{15}\,.\,t' + 24^{\mathrm{h}}\left(1 + \frac{\delta n}{n}\right).\,t',$$

Le Verrier's expression adopted in 1864 was

$$(\gamma)\quad s = a + \frac{n+\delta n}{15}\,.\,t' + 24^{\mathrm{h}}.\,t'.$$

The addition of the term

$$24^{\mathrm{h}}.\,\frac{\delta n}{n}\,.\,t'$$

makes, therefore (γ) identical with (β), and it can, therefore, be re-transferred identically back into (α). This term is, therefore, the necessary correction to make the mean times deduced from Le Verrier's expression identical with those which would have been obtained from the continued use of Bessel's expression. And since the change from Bessel's expression to Le Verrier's in 1864 was an arbitrary act of astronomers, the discordances thus introduced between Hansen's Lunar Tables and observations are the results of our own action, and have nothing whatever to do with physical causes; and what we require to estimate is the *effect of the change*, and this is what I have done.

I am perfectly at a loss to understand the opposition with which my work has been received. It appears to be considered that there is some absolute physical necessity for the use of the true mean solar day as the unit of time in practical astronomy, although such a use is absolutely impossible for the reasons which I have already given.

Questions respecting Mr. Stone's Theory of Changes in the Mean Solar Day. By Prof. Simon Newcomb.

I have refrained from joining in the discussion of Mr. Stone's views respecting the mean solar day found in the *Monthly Notices* of May last for two reasons. One is that the matter seemed too elementary in its character to be the subject of prolonged discussion. The other was a desire to wait until Mr. Stone should give a statement of his views sufficiently explicit to admit of their correctness being tested by practical application. As the discussion has now been going on for the greater part of a year, the first of these reasons has ceased to be operative. At the same time I note with surprise that Mr. Stone still refrains